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voluntarily assured me that the year before she was "nigh *h*'eat *h*'out of 'ouse and 'ome by them sparrows!"

The naturalist traveling through England is much assisted by her museums. Though, after a hasty review of its natural history department, I flattered myself (but then in flattery there is often deception) that the British Museum did not have so fine a collection of birds as has our Boston Society, yet with some of the local museums I know nothing in this country to compare. Salisbury, for instance, a town with no large population, had a museum, which, purposely and wisely local in character, instead of attempting an ostentatious display, confused and vastly incomplete, of a world-wide gathering, had, in its ornithological department, a simple, thorough and admirable collection of British birds, which artistically, taken all together, were the best mounted I had ever seen. What an easy Natural History to read, and what a pleasant guide-book to that neighborhood! At Torquay, if I remember right, the collection was still more local. I wish that our people would be content, outside of their great central museums, to imitate these examples. I fear that they are too eager, ambitious, and fond of show; though perhaps, should I spend five months in traveling through the towns of New England, I might in this matter be agreeably disappointed. To the proper protection of living birds, however, and to the reasonable preservation of Nature, the American people certainly pay no sufficient heed.

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ON THE AGE OF THE LARAMIE FORMATION AS INDICATED BY ITS VEGETABLE REMAINS.

BY J. STARKIE GARDNER.

I SHALL confine myself to such inferences which bear on the question of the age of the Laramie formation, as have been drawn from a consideration of the British Eocene floras alone.

In the first place it is desirable to consider carefully the extent of the gap between the Cretaceous and the Eocene formations in Europe, as it is probable that a portion of the strata whose age in America is disputed, belong to this interval. The discussion itself and all the arguments brought to bear seem to render this solution possible.

The completely different character of the fauna of each, which led the older geologists to draw their most emphasized line of division between them, presupposes of itself a great interval. The conception I have formed of this interval is such, that I should feel no difficulty in referring strata to it, even to a thickness of 10,000 feet. I will endeavor to explain the reasons for this belief.

In England no break could be more complete, geologically and zoölogically, than that between our lowest Eocene and the chalk upon which it rests, although to the eye but a slightly eroded surface and a few flints separate them. The Eocenes are the result of a long series of local deposits of shallow estuaries and rivers, and the causes which led to their deposition can be traced. The chalk contrasts very strongly, being the deposit of wide ocean without any indication whatever of the proximity of land. We have but a mere fragment left, and its former presence over the larger part of Great Britain and Ireland can only be traced by the flints which have been left behind. We do not yet even know what were the shore lines in any direction of the Cretaceous ocean, for the indications of it in Europe may belong to a later period when part of its bed was being elevated into Præeocene land.

No less striking is the completeness of the change which took place in the fauna and flora during the interval. In the Eocenes the progressive change in the Mollusca, notwithstanding the evidently varied physical conditions under which they lived, was so small that they are substantially the same throughout. The changes underwent by them during the whole Cretaceous period is also so small as to be almost entirely of the value now commonly recognized as specific and not generic. When we compare the Cretaceous and Eocene faunas of England together, they seem, however, to have hardly anything in common. In the long interval between them, genera, families and even orders had become extinct, and new types taken their place. We may form some estimate of the lapse of time such changes as these imply, supposing life forms to have been progressively modified, by examining three strata with which I am especially familiar—the Neocomian at Atherfield, the Gault at Folkstone, and the Upper Green sand at Blackdown. These answer the purpose better than any other, because they contain a very similar assemblage of Mollusca, and seem, therefore, to have been deposited by water of approximately the same depth, although the sea bottom was of different natures, and further there may be gaps of age between them. In all these the genera of Mollusca are the same, and the species marvelously similar, yet when compared together they are found in nearly every case to be slightly modified. Here we have merely specific changes of minute extent, although it would be impossible to estimate the thickness of the deposits from Neocomian to the Upper Green sand, inclusive, comprising also Aptien and Gault, at less than one thousand feet, and the probability is that they far exceed this. This is merely an example, but similar beds, either of Jurassic, Cretaceous or Eocene formations, if contrasted together separately, lead to the same result, provided, of course, that those examined were deposited under like conditions.

Now to compare, for a moment, Cretaceous and Eocene strata. The Gault and the London clay are both massive deposits of clay which seem to have been formed under almost similar conditions and on the same area. The utter dissimilarity of the Mollusca of each, however, is such that the time required to have effected such a change (always admitting progressive modification) would have permitted the accumulation of very many times more sediment than the amount which actually separates them. It may be objected that a part of the difference which we see in these littoral faunæ took place, to an unknown extent, during the deposition of the chalk itself, but this does not seem to have been so to any great degree, since but a slightly modified Gault fauna can be traced through the Gray chalk into the lower White chalk, whilst Ammonites and Belemnites are met with in the highest chalk rocks of England without any mixture of Tertiary forms. There is nothing, therefore, to indicate that any extensive modifications had taken place up to the close of the chalk period in England.

Taking the floras of Cretaceous age in England, whose horizons are absolutely known, we see that they point to an even greater interval than the fauna. Of course I leave out of the question the so-called Upper Cretaceous floras of Europe, whose age, not based upon stratigraphical evidence, is even more a matter of doubt than those of America. The Neocomian flora, from the little we know of it, was similar to that of the Wealden, and the Wealden to the Jurassic; plants would seem, therefore, to have become modified even more slowly than animals. My own collecting in the Gault, at Folkstone, for twenty years, during the latter part of which I have employed, constantly, a collector, goes far to show that up to that period dicotyledons did not exist, in these latitudes at least. Five new species of cones, branches of conifers with leaves attached, resin and coniferous wood are found, the latter abundantly, but no dicotyledons. I cannot but believe that had dicotyledons existed, some trace of them would by this time, have been found. The same is true of the Gault elsewhere, and especially in Hainault, where a more abundant flora has been brought to light. Neither in Neocomian, Gault, Upper Green sand or chalk, or any Cretaceous deposit in England, has anything leading to the supposition that dicotyledons were then in existence, yet been found. Dicotyledons may have been developing in other areas at this or an earlier period, especially towards the Poles, but from the evidence of British rocks, I should refuse, in the absence of confirmatory stratigraphical evidence, to assign so great an antiquity as that of our chalk to any deposits containing dicotyledons in our latitudes. If clearly older than our Eocene, I should refer them to the great intervening period.

From the almost complete absence of Cretaceous forms in even the lowest European Tertiaries, it seems to have been concluded everywhere, that all rocks containing even a small proportion of Cretaceous types, must be classed as of that age. Isolated protests have been raised, but their value has not been felt. This basis of classification is, in my opinion, entirely erroneous, or at least carried to an excess, for all we know is, that the fauna which existed in Cretaceous seas did not exist in those of the Eocene. How or when it disappeared from these areas we do not know. The extinction, or perhaps partly migration, must, however, have been a natural and gradual one, and we see in many distant countries, California, New Zealand, India, Vancouver's Land, for instance, that late Cretaceous types of fauna lived long after the time of which we have any record of them here, and mingled with a fauna whose characteristics are decidedly Eocene.

For the reasons already advanced I should regard the flora of Dakota, together with those of Nebraska, Vancouver's Land, New Zealand, and many European floras, characterized by an abundance of dicotyledons, as belonging to a vast intermediate period, and should adopt a name suggested by Hector for it—*Cretaceo-Eocene*—in preference to the term Pal-Eocene, used by

Schimper, the latter having been applied by him to true Eocene floras.

With regard to the flora of the Great (1st group of Lesquereux) Lignite, I entertain no doubt whatever that it is of the age of our *Middle Eocene*, and perhaps partly of our Lower Eocene. I am not in a position yet to furnish any list of the fossil plants common to both, but the proportion is very considerable. The only groups I have studied are the ferns. Of a small list from our Middle Eocene, two of the most abundant have been described by Lesquereux from this formation, *Lygodium kaulfussi* Heer (*L. neuropteroides* Lesq.) and *Osmunda*[1] *(?) subcretacea* Saporta (*Gymnogramma haydeni* Lesq.). Mr. Lesquereux has very generously himself assisted in these identifications, and I desire to express to him my thanks for his disinterested aid.

In addition to the similarity of the floras, there is other strong proof that the two formations are approximately contemporaneous. While in our lower Eocene deposits there appears but a small mixture of North American forms, so far as I know at present, in the Middle Eocene they suddenly greatly preponderate, almost to the exclusion of the Australian element previously manifest, and even of what was possibly an older indigenous flora. Judging as well from the Great Lignitic flora as from our own Middle Eocene, it appears evident that at this period land communication somewhere existed between them, which enabled them to mingle to a very great extent; so much so, indeed, that the Pliocene flora of California, lately described by Lesquereux, more resembles our Middle Eocene Bournemouth flora as a whole, not specifically, than any other with which I am acquainted.

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NOTES ON THE FLOWERING OF SAXIFRAGA SARMENTOSA.

BY PROF. J. E. TODD.

ONE often sees in window-gardens the plant popularly called strawberry geranium. As commonly seen, perched in a flower pot on a bracket, it seems to delight in letting down its young plantlets at the ends of thread-like runners, sounding the airy depths for resting places for them. Thus its native instincts appear, though many generations have passed since its ancestors

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[1] More properly a new genus.